REVIEWS.

TEXAS FEVER.

Southern Cattle Plague (Texas Fever).—An Etiological Study, by Frank S. Billings, Lincoln, Nebraska, U.S.A.

In a book of 138 pages the disease known here under the above title is treated of by Mr. Frank S. Billings, Director of the Patho-Biological Laboratory of the State University of Nebraska. On the title page we find that the work is "An Etiological Study," and that the author prefers to call the disease, The Southern Cattle Plague of the United States.

"The nomenclature of this disease" says Mr. Billings in his opening sentence, "has been as varied as the ignorance of people about it." It has been called, "Splenic Fever," "Texas Fever," "Spanish Fever," and other names, which so exasperates our exact and scientific author, that he is compelled to express himself strongly. He says: "The word 'fever' to express any peculiar characteristic of any given disease is a misnomer, illogical and unpathological. Its continued use in this respect is one of the most lamentable phenomena in connection with medicine. It shows the most abject ignorance of every principle of pathological philosophy." Now the old names were not given to "express any peculiar characteristic," but simply to name or identify a certain disease, just as the term "yellow fever" is used. Why Mr. Billings should have thought it necessary to write this rhetorical and inapplicable sentence, we do not see, and more especially so, as he uses the name "yellow fever" without apology.

We gather that Mr. Gamgee reported on this disease in 1871, and attempted some definition of it. Quotations are given from his report, and the following criticisms made thereon. "More illogical and absurd contradictions than the above could not possibly emanate from the merest novice upon disease," and "Further quotations need not be made to show that Mr. John Gamgee did not know what he was writing about when he said, &c." Well, possibly this is all true. In 1871 very few people knew much about Texas Fever, and we are not quite sure that some opinions now held will be verified in the light of scientific knowledge 17 years hence. In 1871, we, in this old country, did not look upon John Gamgee as "a novice upon disease," nor did we often find that "he did not know what he was writing about." In 1883, Mr. D. E. Salmon had the misfortune to write a report on this subject, and Mr. Frank Billings says: "he is certainly as full of contradictions and uncertainty as to its nature as Mr. Gamgee." Nobody, in fact, knows anything about its nature except our author, who clearly settles the point thus—"the only logical conclusion as to this disease is, that it is an Extra-Organismal-Infectious-Septicœmia." What a grand thing it is to KNOW!

As to the Geographical Distribution, we are told that so far as is known in the United States, this bovine pest is peculiarly an American disease, and not known to exist in any part of Europe, Asia, or Africa. Quoting an account from the "Veterinary Journal" of a disease which lately appeared in Italy, Mr. Billings remarks: "it looks very much as if the disease mentioned were this plague, or a near relative," and he indicates a number of points of analogy between them.

In America, the disease is not universal, "it finds its original development in a limited portion of the United States,"—in those States which border upon the Gulf of Mexico, or the Southern Atlantic Ocean. In these States it is permanent, but unfortunately, the territory of permanent infection is extending, and some statements in the Annual Report of the United States Department of Agriculture show that the infected area extends at the rate of about four or five miles a year.

The next section of the book treats of the History of the disease, from which we gather that "Texas fever" can be distinctly traced back to the eighteenth century. How long it may have existed in Texas and the Gulf States is not known, but it came northward with the first cattle driven from the country bordering on the Mexican Gulf, and infected cattle along the line of march. The first notice of the disease is in a lecture delivered by a Dr. James Mease in 1814, in which it was mentioned, that "the South Carolina cattle had the power of infecting others with which they came in contact, while they themselves were in perfect health (apparently). As rail and steam communication progressed, so the plague extended more widely and more rapidly. In 1868, it spread rapidly, reaching Ohio, New York, New Jersey, and as far east as New England. At this time the Agricultural Department availed itself of the services of the then eminent veterinarian, Mr. John Gamgee, whose investigations upon the bovine scourge, Mr. Billings confesses, "are among the best that we have."

Stringent regulations prohibiting the importation and movement of southern cattle were adopted, and for awhile restricted the traffic in these disease-bearing animals. Of late, however, the trade has increased, and the disease has again spread, especially during the summer of 1887. The idea that the Texan cattle that brought the disease northwards with them were not themselves diseased, is not correct. In Texas there are parts free from disease, but in the infected districts most of the cattle suffer from a latent or mild form of affection, which is only detected on a post-mortem examination.

These animals are capable of contaminating pastures on which they graze. In its primary generation the disease is one of an absolutely local character, and it retains this peculiarity when imported to the Northern States. The Southern cattle are not so much the *cause* of the plague in northern stock as the *vehicles* of infective matter conveyed by them in their excreta. Their power to spread disease seems to depend greatly upon the time which has elapsed since they left the native grounds of infection. That Northern infected cattle do not spread the disease is untrue, and a number of cases

are given by Mr. Billings, in which native cattle infected by Texan beasts have in turn infected the pastures, and through them other native cattle.

The infective material seems to be carried by the urine and fœces. The period of incubation is about 15 days, but there is a period of latency, so that deaths are not seen for a considerably longer time than this after the diseased cattle have been in contact, or have passed over the pastures of the contaminated herd. After Texan cattle arrive on a pasture, no effects of the infection are seen amongst the native stock till about 50 days, when the disease appears.

Mr. Billings next proceeds to discuss the specific cause—the "causa sufficiens" of the Southern cattle plague. Referring to the results of previous work, he first contradicts the hypothesis of Dr. H. J. Detmers—that the saliva is the chief source through which infection reaches a pasture, and fairly argues that the urine and fœces are positively the channel through which the infective element reaches the ground. He also gives reasons for the statement that frost will kill the germs if the cold come into direct contact with them. In Texas, experience shows that germs on the surface are killed in winter, but that those that are deeper in the earth, and so protected from the extreme cold, are not killed; which fact is offered as an explanation of the cessation of disease during the winter and its recurrence in the spring.

As to the identification of the germ, Dr. Stiles in 1867, arrived at the conclusion that it was a micrococcus, and makes the remark, "that the fungus origin of zymotic diseases is now conceded by the highest authorities in mycological research." This was contradicted by Dr. John S. Billings in a report to the United States Government in 1871, and our author points out that Dr. Stiles was correct in his generalisation, though not in his specific assertion. The next bacteriological investigation was made by Dr. Salmon in 1883, who states that the specific germ was a diplococcus. This Mr. Billing denies, after discussing Dr. Salmon's work and quoting various passage which at least warrant the statement that his results were merely negative. Mr. Billings then claims to have found the germ beyond dispute, and in answer to the question—how may we know that we have discovered the germ in any specific disease, lays down three conditions which he claims to have fulfilled in his own investigations. These are:—

First.—In the tissues of animals ill with a specific disease must, in each case examined, be found the same germ.

Second.—This germ must be cultivated, free from every other germ in some artificial media.

Third.—It must be shown that the germ in question has pathogenetic qualities, by inoculating animals and killing them thereby.

Not only have these conditions been fulfilled, but cattle have been inoculated from the cultivated solutions, "and the Southern cattle plague produced in those cattle." This is the positive proof that the germ cultivated is the specific germ. Drawings are given of the germs so discovered, and Mr. Billings says:—"These micro-organisms are neither to be classed with micrococci or bacilli. They are not round objects like the former, or rods like the latter. They belong to that intermediate group, to which, for convenience sake, patho-bacteriologists are beginning to give the name "bacteria," which, while perhaps not a scientific classification, has many practical reasons in its favor.

Mr. Billings then devotes a great deal of space to a comparison between Southern Cattle Plague and Yellow Fever, and certainly makes out a strong case in favor of a very close analogy. The germ found in each is similar, the morbid changes produced are similar, their origin and spread are alike. He concludes then, that "Southern cattle plague and yellow fever are Extra-Organismal-Infectious-Septicœmia. By this, we mean, that they are septicœmia produced by germs that find their primary origin entirely outside of the animal organism."

Mr. Billings concludes his book with suggestions for prevention, which amount shortly to—1st, the quarantining of all Southern cattle at some given point outside of the most northern limit where the disease exists, for a period of thirty days; and 2nd, thorough disinfection of all manure or objects which may carry infection.

He also advises inoculation in those districts where the disease may, etiologically, be called at home.

We have no hesitation in saying that Mr. Frank S. Billings has made a valuable contribution to science, that he is a careful and skilled bacteriologist, and may be expected to do more good work, but we feel sure his opinions would be more readily accepted, and his rank and ability acknowledged with less hesitation, if he would cultivate a less aggressive mode of expression, and be a little more charitable to the honest endeavours of other observers.

EXTRACTS.

POISONS AND POISONING.

A Lecture delivered by C. Meymott Tidy, Esq., M.B., etc., before the Royal Institution of Great Britain.

Toxicology is the science of "poisons and poisoning." How comes "toxicology" to mean "the science of poisons?" The Greek word TOXON signified primarily "a bow." In the very earliest authors, however, it included within its meaning "the arrow shot from the bow."

In the first century, A.D., Dioscorides, a Greek writer on Materia Medica, uses the expression TO TOXIKON to signify "the poison *for smearing arrows with.*" Thus, by giving an enlarged sense to the word we get our modern and significant expression "Toxicology," the science of poisons and poisoning.

The history of poisons and poisoning, forms a tempting subject for my lecture to-night. The

histories of Circe and Medea—of Tiberius and Nero—of the Borgias—of Catherine de Medicis, and a host of other worthies, have proved charming topics for the marvel-monger. But this is not the subject I have proposed to myself for the discourse to-night.

"WHAT IS A POISON?"

Two difficulties present themselves in answering this question:—

1. *The law has not defined a poison, notwithstanding that the law at times demands of science the definition of a poison.*

2. *The popular definition of a poison is far from being a sound, much less a scientific definition.* Generally speaking, it comes to this, that "A poison is a drug that kills rapidly when administered in a small quantity."

The phrase "a small quantity" as regards weight and the word "rapidly" as regards time, are as indefinite as the classical piece of chalk as regards size.

I define a poison as—

"Any substance which otherwise than by the agency of heat or electricity is capable of destroying life, either by chemical action on the tissues of the living body, or by physiological action after absorption into the living system."

My definition limits the mechanism whereby the toxic effect is induced, to *chemical* and *physiological* actions. I am conscious that this definition suggests classification. Certain it is, that Nature hates classification as truly as she declines definitions.

Let us trace some of these mechanisms of toxic activity. I select three illustrations of poisons belonging to different classes.

(1) *Sulphuric Acid.*—If a person swallows *sulphuric acid,* the tissues with which the acid comes into contact are more or less charred—"more or less," that is, according to the strength of the acid and the time of contact.

Charred.—This implies a chemical act, dependent on the power of sulphuric acid to combine with water.

The portion of the body thus charred dies. We call this *molecular* death. Before long all the members suffer with the charred stomach. The death, localised in the first instance, becomes general, the death of the entire body eventually taking place. We call that *somatic* death. The primary act of disturbance resulted from the chemical power of sulphuric acid to combine with water.

(2) *Carbonic Oxide.*—Carbonic oxide is a true poison.

Its importance amongst poisonous bodies depends on the circumstance that it is evolved in many manufacturing operations.

What, then, is the mechanism whereby carbonic oxide destroys life? The active agent of the blood is its red colouring matter. We have reason to believe that hæmoglobin is formed from the albumenoids, the synthesis of which albumenoids is limited to the vegetable. Essential as the albumenoids are to animal life, the animal is dependent for their formation on the synthetical processes taking place in the plant laboratory. The animal, however, can transmute one albumenoid into another (*e.g.*, he can change albumen into a peptone), whilst he can also form from them bodies of less complicated constitution (*e.g.*, fat)—in other words, he can lower them in the scale. But, save with one exception, he cannot use them to effect higher synthetical formations. This single exception is hæmoglobin.

It is no matter for surprise that a body like hæmoglobin should possess unique chemical properties and relationships. I shall only trouble you with one of these relationships.

As a general rule a substance that combines with oxygen with difficulty parts from it with ease, and *vice versa.* It is difficult to make gold combine with oxygen, but it is easy to decompose oxide of gold. Potassium easily combines with oxygen, but it requires the genius of a Davy to separate potassium and oxygen.

In hæmoglobin, however, we have a substance that combines with, and delivers up, its oxygen (*i.e.*, is oxidised and reduced) with almost equal facility under similar conditions. Upon this and other chemical characteristics of hæmoglobin, the act of living depends. In other words, life depends on *the integrity* of the hæmoglobin—on the rhythmicity of those chemical processes in effecting which hæmoglobin is the primary worker.

With these facts before us, let us turn to the toxic action of carbonic oxide.

The hæmoglobin at once seizes upon and combines with the carbonic oxide, carbonic-oxide-hæmoglobin being formed.

Two difficulties arise:

1. The hæmoglobin, saturated with carbonic oxide, cannot combine with oxygen.

2. The hæmoglobin, being saturated with carbonic oxide, cannot get rid of the carbonic oxide under the ordinary conditions of respiration and circulation.

To put all this in scientific phraseology, carbonic-oxide hæmoglobin is a comparatively stable compound, being neither decomposed by the presence of an excess of oxygen (as in the lungs) nor by carbonic acid. Death occurs because the integrity of the hæmoglobin has been disturbed—because the normal sequence of its oxidation and reduction has been interrupted by the formation of carbonic oxide hæmoglobin.

(3) *Strychnine.*

How does strychnine act? We know sadly little about it—so little that we use the phrase "physiological action" to express our want of knowledge. But we know something.

A marked chemical characteristic of strychnine is its power to combine with oxygen when the oxygen is presented to it in a nascent form.

Note then the conditions. Strychnine is in the body. There is also present in the blood hæmoglobin loosely combined with oxygen, which oxygen the hæmoglobin is always ready to give up on first demand. We are able to trace this action, and to see that the period of the primary strychnine fit coincides with the reduction of the hæmoglobin.

Why does that kill? I cannot tell you.

My object has been to trace toxic energy to the

chemical action of poisons on living tissues or fluids. The phrase "physiological action" must not be understood as implying any theory *re modus operandi*. There is a danger lest the phrase "physiological action" should be employed, or regarded, as explanatory. It no more explains the action of certain drugs on the living body than the word catalysis explains fermentation.

INSTINCT AND REASON.

What is the distinction between man and his so-called "poor relations?" In nine cases out of ten an ordinary educated man will reply—"well, you see a man has reason, and brutes have only instinct." But if we go on to enquire what is instinct, and wherein does it differ from reason, our friend is at a loss, and if disposed to be candid, confesses that "instinct" is a cabalistic word which we use to cover our want of knowledge.

It is, of course, admitted that all animated beings, at least when mature, show a practical acquaintance with the natural functions of their own voluntary organs. Certain writers, from time to time, profess a very gratuitous wonder that a herring never tries to fly or to burrow in the ground. We should feel much more surprised if the fish in question did make an attempt so out of keeping with the structure and the habits which it has inherited from its ancestors. It is granted on all hands that animals perform certain actions primarily needful for the well-being of the individual or the species. These actions are termed by some (G. H. Lewes) "organic," and by others "instinctive." It is next assumed by authors, though scarcely upon sufficient grounds, that in the lower animal such actions are executed without any previous knowledge, training, or experience; without reflection; without any prevision of the results; without any necessary reason or consciousness; and without evincing a "designed adaptation" of means to ends. An additional step is then taken, and we group together all these sections and imagine them as being prompted and guided by some mysterious unitary power or faculty called "instinct"—differing from reason, and yet capable to some extent of serving as its substitute.

We must ask firstly for a definition, or in default, for at least an explanation of the term instinct. We then demand by what tests instinctive actions may be recognised, and clearly distinguished from such actions as are performed under the control of reason? Admitting, further, the existence of instincts, we ask are they peculiar to the lower animals, or are they shared by the human species?

Definitions of instincts have been attempted, though with no very decided success. G. H. Schneider writes: "By instinct we understand the impulse to an action, the purpose of which is unknown to the individual, but which still leads to such purpose."

Mr. A. R. Wallace explains it as "the performance by an animal of complex acts, absolutely without instruction or previously acquired knowledge."

Darwin declined attempting any definition of instinct, but he remarks:—"Every one understands what is meant when it is said that instinct impels the cuckoo to migrate, or to lay her eggs in other birds' nests. An action which we ourselves require experience to enable us to perform, when performed by an animal is usually said to be instinctive."

Lastly Mr. Herbert Spencer hits the mark best when he considers instincts as "organised and inherited habits." and again as "compound reflex actions."

The faculty which prompts to instinctive actions is admitted to be mental, nor can we conceive of it as otherwise unless we regard animals as automatons. One of the definitions ascribes to them the power of performing actions such as in man results from a chain of reasoning. If then, certain actions in man are due to a chain of reasoning, why do we not ascribe the same or analogous actions in the lower animals to the same source? To do otherwise, to seek for some unknown and mysterious faculty in order to explain what may obviously be due to reason is illogical. But some brutes are observed, or at least are supposed, to do things which man cannot perform, and does not know how to set about. For the explanation of such cases "instinct" might be provisionally invoked by sober thinkers as an unknown power.

In thus acting there can be nothing unphilosophical. But we are bound to find out, if possible, what is this unknown power. We must ascribe to it no more than is plainly needful. We must note, too, that the instinctive shrinks up and draws back on close examination. In other words, many actions which were once called instinctive are now found traceable to instruction, imitation, or to knowledge otherwise acquired, or, finally, to the teachings of reason.

It has also been shown that much of what, in the actions of the lower animals, seems to us inexplicable is due to the greater acuteness of their senses, or, in case of insects, to their possession of senses, other, or even more, than our own. That many vertebrate animals far surpass us in the delicacy of their sight, smell, or hearing, is no longer a matter under discussion. As for insects, a variety of points connected with their structure, and still more, a number of observations upon their conduct, almost compel us to admit that they possess inlets of knowledge of whose nature we can form no direct conception.

Within the domain of sight there are regions closed against us. The retina of the human eye is sensitive to a portion only of the sun's rays. If we throw the solar spectrum upon a screen, we see the seven so-called spectral colours in their usual order. But above and below these bands of colour there are radiations whose presence we can show by appropriate physical and chemical means, but which, to our organ of sight, are simply darkness. If the eyes of any creature—or its entire frame—be sensitive to the whole or to a part of these "dark rays," as we call them, such creature will be able to see where we find total darkness, and to see illuminated objects as we do not see them. If, then, certain

animals possess more acute and perhaps more numerous senses than we do, they are in a condition to acquire by direct perception knowledge which we can gain only by trains of inductive reasoning, and by the use of instruments of precision. Very probably most of those instances of alleged instinct which are not resolvable into hereditary habit depend on a simple following of the guidance of senses more acute than our own. We must never forget that to all animals, and not less to uncivilised man, every strange object is unpleasant, and is either attacked or fled from according to circumstances.

That acquired habits may become hereditary, reappearing without any special instruction or example in succeeding generations, is shown by such familiar instances as the pointer, the retriever, and the sheep-dog. Birds' nests have been vociferously appealed to as instances of a mysterious instinct. Say the rhetoricians :—"It cannot be imitation, for, though you hatch a crow under a hen and never let it see any of the works of its kind, the nest it makes will be the same, to the laying of a stick, with all the other nests of the same species. It cannot be reason; for were animals endowed with it *to as great a degree as man* (note the sophism!), their buildings would be as different as ours, according to the different conveniences they would propose to themselves."

This sounds, to the non-naturalist, very plausible. But we are perfectly warranted in referring the general similarity of the nests of any one species to hereditary habit.

Further, as Mr. A. R. Wallace has ably shown, the habitations of birds are modified according to circumstances. We even learn that the European sparrow, introduced into North America, builds itself there a winter-nest for shelter during the inclement season, quite different from its breeding-nest. On the other hand, man, in the construction of his dwellings and in many other matters, often evinces an adherence to hereditary customs, little in accordance with reason.

Perhaps it will be found that the finishing stroke to the doctrine of instinct, viewed as a mysterious substitute for reason, has been dealt by the elder Darwin, and has again been brought under notice by Mr. Samuel Butler. These writers pronounce instinct to be simply "inherited memory." Each animal is simply a continuation, or, as Dr. Darwin expressed it, a "prolongation," of the life of its parents. It has a latent memory of their habits and actions, which may be called into play by the stimulus of circumstances.—*Scientific News.*

VITALITY OF ORGANISMS.

The fungus *Mucor imperceptibilis* flourishes in arsenical solutions, as does also a species closely allied to *Leptomitus*. The mould *Aspergillus niger* perishes in water containing one six-hundred-thousandth of nitrate of silver, and it cannot live unless zinc is present to the extent of at least one fifty-thousandth.

VETERINARY JURISPRUDENCE.

IMPROPERLY FIRING A HORSE.

At the Hove Police Court yesterday, before Mr. M. D. Scott (in the chair) and other Magistrates, "John Strand," of Chapel Mews, Waterloo Street, Hove, appeared to an adjourned summons charging him on remand with cruelty to a horse by firing the same at Hove, on the 13th ult.—Mr. Colam appeared for the Royal Society for the Prevention of Cruelty to Animals, and Mr. J. K. Nye appeared for the defendant. Mr. Colam, in opening, stated that the local Inspector for the Society stopped a certain horse in the streets, and some time afterwards went to the defendant and told him he wanted to see it. Defendant then told him he had fired it, and on the Inspector examining it he found that it had been fired back and front and up and down, and really in such a way as to leave scarcely a sound piece of flesh. Notwithstanding this fact it was a case in which firing was not necessary at all, and he should prove by independent witnesses that there had been great cruelty in this case. Inspector Blake Jones, R.S.P.C.A., stated that on 20th August he called at the defendant's surgery in company with Mr. Spinks veterinary surgeon. He stated that he was appearing against a man named Bletsoe, who had been working a horse in an unfit state at Lewes, and he wanted to see the horse. He said "Yes, the horse is here; I have fired it since you stopped it." In reply to his enquiry as to when he fired it, he said "This day week." That would be Monday, the 13th, and witness had stopped the horse two days before. Defendant brought the animal out from the stable into the yard, and he saw that it had been fired from just below the knee, the marks made by the iron running round and round the leg, and also being crossed so as to form a net-work. The limb was very much swollen up to the knee, and in a most painful condition in consequence of the firing. In reply to the Magistrates, the Inspector said that on the Lewes racecourse he saw the horse swinging its leg off the ground, and it was apparently in such pain it could not put it down. He refused to let it be worked any longer. He told the defendant that he should report the case to his head-quarters, and the defendant replied, "The reason I fired it was that the knee was a bit enlarged and the tendons swollen." Cross-examined by Mr. J. K. Nye, the Inspector was positive that it was on the Saturday that he stopped it, and not on the Friday. Mr. Richard Lewin Spinks, M.R.C.V.S., said the horse was pointed out to him on the 20th August. He found that the horse was about fourteen years old, and in fairly good condition, but suffering pain in the leg. The limb was fired in a net-work fashion. It was unprofessionally done, because it was not necessary to fire in front of the bone. In his opinion sufficient time had not been allowed for the inflammation to subside. He thought the action was unwarrantable and unnecessary. —Mr. Alfred Maller, a registered veterinary surgeon, of Hove, said he treated the horse in June, when it was suffering, amongst other things, from

a diseased foot and disease of the sessamoid bones. The disease was too chronic for firing.—In answer to Mr. Nye, he said that when he saw the horse on the 22nd of June it was in fairly good condition.—Mr. Bernard Wood, M.R.C.V.S., stated that he saw the horse on the 23rd August. It had been severely fired, back and front, from the knee to the foot. He had never known a professional man to fire back and front. On the 23rd, the inflammation was very intense. The horse was not in a proper condition to be fired.—Cross-examined: He had known the defendant for ten years. He did not know that he knew a great deal about horses.—Mr. John Hammond, M.R.C.V.S., stated that the horse had been excessively fired, even if firing was necessary at all.—Mr. W. Hunting, Fellow of the Royal College of Veterinary Surgeons, stated that he had been in practice many years. He thought it would have been advisable for the defendant not to have adopted the diamond pattern firing, which was apt to cause sloughing of the skin. The operation was excessive and unnecessary—extending as it did from knee to foot, when only the fetlock joint was diseased. It was done too soon after cessation of work when great lameness and acute inflammation existed. The horse should have been rested some days and the leg allowed to have become cool and less painful before the adoption of firing. In cross-examination, he said he had seen the horse and he did not think that permanent harm resulted from the diamond firing in this case. He thought that defendant had met with tolerable success in his operation.—Re-examined. The animal was put to an unnecessary amount of pain by the way in which it was fired.—By General Shute: This extra pain might have been inflicted through the ignorance of the defendant.—By Mr. R. A. Bevan: He thought the horse was fired too soon after it had ceased to work.—Mr. Nye, in addressing the Court for the defence, pointed out that many men who were not qualified veterinary surgeons practised as horse doctors, and were as capable of performing certain operations as those who held diplomas from the Royal College. He pointed out that his client had enjoyed a good reputation for many years, and contended that there was not the slightest evidence to prove that the defendant had knowingly behaved cruelly towards the animal. He pointed out there was nothing to show that the defendant thought the horse was not in a proper condition to receive the irons, and said it was not likely that he would wilfully fire the animal in a wrongful way, as his own reputation would be at stake, and the owner of the horse would of course refuse to pay him and want to know why he had damaged the horse. Mr. Strand had a good reputation at Canterbury, and he was also for years in the stables of Mr. Gibson, who owned very valuable horsses at Pulborough, and who gave him the highest testimonial as one fitted to have the care of horses, and he would back this up by the testimony of Mr. Young, who owned a very large number of horses at Brighton. The defendant was a good friend to the Brighton cabmen, and he would suggest that this case was simply an attempt to blot him out because he did not hold a diploma as a veterinary surgeon.—Henry Bletsoe the owner of the horse, stated that he took it home on the Friday and put cold water bandages on it. This was continued until the Monday, when it was fired. He thought the horse was cured. It went well now, and one could hardly see that it had been fired. On the day it was fired the leg had gone down to the size it was now.—Cross-examined, he was quite sure that the inflammation of the leg had gone down on the Monday. It was going down on the Sunday morning.—Mr. Samuel Young, livery stable keeper, of Brighton, stated that he had been used to horses all his life. He now had more than two hundred horse. He had known the defendant for fifteen years, and knew that he had all Mr. Gibson's valuable racehorses under his care. He had fired several horses for witness with good results. He considered him capable of firing horses, and believed he was a humane man. He saw the horse said to have been ill-treated two or three days after it was fired, and the state of the leg was consistent with its having been properly fired. The horse was not severely fired or the hair would not have grown as it had.—The Bench retired for private consultation, and on their return into court the Chairman said they had given their best consideration to the case, and a majority were of opinion that the defendant should be convicted, and fined 5s. and costs.—Mr. Nye asked if the Bench would make it 40s. and costs, so that the defendant might appeal, and the Chairman replied in the affirmative.

NOTES AND NEWS.

The Veterinary Colleges.

Next week the winter session commences at all four of our teaching schools.

On Monday October 1st, the Royal Veterinary College, London, will open. The Chair will be occupied by Dr. Fleming, P.V.A., and at one o'clock Professor Brown, C.B., will deliver the Introductory Lecture.

On Wednesday October 3rd, the Royal (Dick's) Veterinary College, Edinburgh, will open their 66th winter session. The invitation card from the Lord Provost, Magistrates, and Council of the city of Edinburgh fixes the time at two o'clock, when Professor Walley will deliver the Introductory Address.

On Tuesday October 2nd, the Glasgow Veterinary College commence their session. The Introductory Address will be delivered at three o'clock p.m., by Professor Cooke, F.C.S., in the College buildings, Buccleugh Street. Thomas Morrison, LL.D., Rector, Free Church College, in the Chair.

On Wednesday October 3rd, the New Veterinary College, Edinburgh, also commences its session. The Chair will be taken by the Right Hon. Earl of Haddington, and Professor Ivison Macadam will deliver the Introductory Address.

Nothing pleases the Principals of all the Schools more than to see their old pupils and professional friends come in force on the opening day, to honour their Alma Mater.

Royal Scottish Veterinary Society.

A Meeting of the above Society will be held on Wednesday, the 3rd October, at three o'clock, in the Dick College Buildings, Clyde Street.

BUSINESS :—

1. Minutes of last Meeting.
2. Election of New Members.
3. Discussion on the Report of the Departmental Committee on Pleuro-pneumonia and Tuberculosis.
4. Remarks on interesting cases, with specimens, by one or two members.

W. D. FAIRBAIRN, Hon. Sec.

Fatalities in the Profession.

On September 6th Mr. B. R. Kirk of Huddersfield, was found dead in his room. The body was partially burnt, and a spirit-lamp was found overturned under it. It is supposed that the deceased had fallen with the lamp and being insensible was suffocated. Mr. Kirk obtained his diploma in Edinburgh in 1863, and had rather a chequered career. He acted as Veterinary Surgeon in the Federal Army during the American Civil War, and afterwards practised for a short time in the States. He also practised at Huddersfield, Bakewell, London, and for a short time in Paris. He had returned to Huddersfield and resumed practice there about two years, when the sad accident occurred which caused his death.

On September 22nd Mr. Frederick T. Welham, M.R.C.V.S., practising at Brentwood, Essex, died under very painful circumstances. He returned home about mid-day from attending some cases, and feeling unwell he laid down and endeavoured to get to sleep. Failing to do so he drank some chloral, for the purpose, it is supposed, of inducing sleep, but, unfortunately, he took an over-dose. He at once became conscious of this and called his wife, telling her he had poisoned himself, and asking her to get him some oil and water. Mrs. Welham administered some oil, but, although it produced vomiting, it failed to have the effect desired, and death ensued in a few minutes. The deceased was 41 years of age, and leaves a family of six young children.

At the inquest held on Monday, Dr. Burgess, who had made a post-mortem examination, attributed death to shock, acting on a diseased and weak heart, upon the deceased discovering that he had taken an overdose of chloral. The chloral would not have produced death so quickly.

Anæstheticism in China.

Says *La Nature*, it is often remarked that all our discoveries, even the most wonderful, have seen the day in China long before they became known in Europe. Thus the use of anæsthetics was familiar in that country before it was taken up by American surgeons. A celebrated Chinese physician living in the 13th century seems to have discovered the use of anæsthetics in operations. He made use of a preparation of hemp which in a few moments rendered the patient absolutely insensible.

Not an Amateur.

There is a story going about so good that it ought to be made public, and so improbable that it must be true. A very eminent London surgeon —one of the lights of the profession—the other day observed a gentleman fall in the street. He went to his aid, and found he had broken his leg. It was only a simple fracture, but the man was badly hurt. The surgeon used his umbrella as a splint, and with his own and borrowed handkerchiefs bandaged the limb tightly, put the patient in a cab, and drove to the nearest hospital. There they were received by the young surgeon, or his *locum tenens*. "You've bandaged this very well," said the hospital surgeon. "You flatter me," said the great gun. "Not at all," said the other—"I suppose you've been attending an ambulance class. They say a little learning is a dangerous thing, but the little you've learnt you've put to good account. I can't give you your umbrella now, but if you leave your address it shall be sent home." "I had best give you my card," said the eminent surgeon. And he did so.

Animal diseases in Egypt.

Writing from Egypt, the *Lancet* Correspondent says: "Probably for the first time in the history of the Country, the Public Health branch of the Government has issued a report."

It relates chiefly, of course, to the diseases of man and the provisions made to prevent them. One paragraph is of sufficient interest to veterinarians to quote; it is under the head :—

VETERINARY SERVICE.

"The first authentic case of rabies occurred in Egypt in August, 1886, and in the following January a second case was found in an English dog in Cairo; while in June, 1887, a third case was reported from a village near Cairo, the dog having bitten eleven individuals, three of whom are said to have died of hydrophobia. This small record of rabies is very satisfactory, when it is remembered that Egypt contains thousands of dogs in a half-savage condition.

During the past year there were eighteen cases reported, and probably others unreported, of glanders and farcy, but none communicated to man.

Pleuro-pneumonia occurred among buffaloes and bullocks in three different provinces, but was easily checked by the sanitary measures adopted."

CORRESPONDENCE.

ROARING IN HORSES.

SIR,

I am exceedingly sorry Mr. Jones of Leicester did not attend the annual meeting of the National Veterinary Association, for he would have seen what he says he would gladly come all the way to Newcastle to see repeated; that is, to behold a horse anæstheticised by 1½-oz. of chloroform. The first animal Professor Smith operated upon here was a chestnut mare with a foal at foot, she had administered to her an ounce and a half of chloroform, which rendered her perfectly insensible, and was found to be quite sufficient to

keep her so during the time the operation lasted. So you see it is no very exceptional circumstance, as Mr. Jones thinks, for a horse to be rendered unconscious with 1½-oz. of the anæsthetic.

The second horse Professor Smith so ably demonstrated upon was a bay horse. He took 3-oz. of chloroform to render him devoid of feeling, but it was apparent to most present that he had inhaled more than was beneficial for him, as he took more reviving and was a longer time than the mare before he was able to walk to his box. Had I not had two veterinary surgeons to assist me in operating upon the horse, one of whom, Mr. Andrew Hunter, administered the chloroform, I have no doubt Mr. Jones would have said my statement was an exaggerated one. Mr. Jones greatly *astonishes* me (if it does not justify a stronger term) when he says he has known a horse inhale nearly three pints of chloroform in the course of an operation, and take an hour to become fully under its influence. I would like to know what kind of an operation that poor horse underwent, and who was the administrator of the chloroform. Certainly he must have been such a very nervous individual as no amount of practice or experience will ever make a good operator. I cannot believe the horse ever inhaled the whole of the three pints of chloroform, three-fourths of it must have escaped or been wasted. But if it be correct, and Mr. Jones was the administrator, then I think although he has had considerable experience in its administration, yet he needs a few private lessons, which we will be glad to give gratuitously.

Then as to the bleeding when opening the larynx; if he takes Professor Smith's advice he will not open that organ until hœmorrhage from division of the muscular tissue has ceased, but if the muscles are torn and not divided with the knife, Mr. Jones will find little if any bleeding in any horse, for in the inferior and central line of the larynx we have no important or large blood vessels to trouble us, as in Dr. Clarke's lateral operation where the jugular vein and laryngeal branch of the thyroid artery have to be ligatured. Then as to Mr. Jones' prophecy. I expect the very reverse of what he predicts. But why does he prophecy? Does he think I did not perform the operation in a skilful manner? I only know that I succeeded in doing what I attempted to do very easily. But after taking Mr. Hunting's advice, and reading Dr. Clarke's very able and instructive paper in the August number of *The Illustrated Military Magazine* I think the operation I performed of excising the left vocal chord and the base of the arytenoid cartilage can be greatly improved upon. Although the horses operated upon at Newcastle-upon-Tyne are not a success, yet I have good hopes, as Dr. Clarke shows us, to be able to fix the left side of the larynx in a state of complete abduction, so that a horse which has been a bad roarer may, after being operated upon, make little or no noise when galloped.

Yours faithfully,

WM. HUNTER.

Westgate Road,
Newcastle-upon-Tyne.

HOW MUCH CHLOROFORM IS REQUIRED TO ANÆSTHETIZE A HORSE?

Dear Sir,

This question is suggested by some remarks made by Mr. Jones, of Leicester, in your issue of September 15th inst., wherein a doubt is expressed that ʒiss of chloroform is sufficient to anæsthetize a horse.

The quantity of chloroform vapour required to produce insensibility and the rapidity of its action, depends very largely upon the percentage admixture of atmospheric air inhaled with it. Following the generally recognised method of allowing a large admixture of air, the average quantity required is much higher than by Mr. Robert's method of allowing only a limited dilution. By this latter method, which was introduced to the profession by a paper read at a meeting of the Border Counties Veterinary Society in the autumn of 1884, 1½ to 2 ozs. of chloroform is the *average* quantity required to produce complete anæsthesia. Since then, it has been largely and successfully employed by many practitioners in this county. Personally, I much prefer it to the old method, and to those who have not employed or seen it used, I would advise to lay to one side any fear or misgivings that they may have and give it a fair trial. The theoretical dangers are not borne out by practical experience.

Mr. Carlisle has modified the original chloroform bag, so that it more thoroughly answers the ideas of Mr. Roberts.

Yours truly,

J. DONALD.

Wigton, Cumberland.—Sept. 23rd, 1888.

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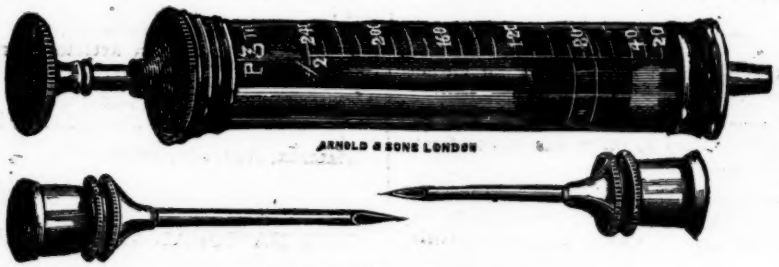

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Extract from THE VETERINARY JOURNAL.

"Acting on this suggestion I have treated a large number of calves in the same way, and in almost every case with success; instead of Tr. opii, however, I have substituted chloroform, ʒss., which I know to be a deadly parasiticide. The operation is very simple and easy to perform. I procured a large syringe for the purpose from Messrs. Arnold and Sons, the needle of which I introduce about midway down the cervical portion of the trachea, without regard to an inter-annular space, and inject the fluid slowly. The exhalation of the turpentine can at once be detected at the nostril: the animal appears to suffer no inconvenience beyond a slight cough. So far, I have not found it necessary to repeat the operation, although some of my cases were apparently suffering from advanced Broncho-pneumonia."

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